October 8, 1952

Dr. Joseph Stasney
Department of Pathology
Jefferson Medical College
Philadelphia, Pa.

Dear Dr. Stasney:

Thank you for sending me a reprint of your 1950 paper on the "transduction" of neoplasia via chromatin fractions. From your recently published contribution to the Conference on Viruses and Cancer, published by the N. Y. Acad. Sci., I would conclude that the problem continues under active investigation.

In my previous letter, I wondered whether the action of DNAse might not be used to rule out cellular contamination as the basis of the transfer. For technical reasons, I can see how this might be difficult to apply in practice. May I be so bold as to suggest another type of experiment which should be entirely feasible, and which would be a rather provocative contribution to the genetics of somatic cells [the aspect that happens to hold my own particular interest].

As I may have mentioned, our own work on genetic transduction in Salmonella would seem to be based essentially on the same mechanism that you postulate for your material. We have been fortunate in the technical advantages of our material, so that genetic studies have been pursued with a minimum of difficulty. We have consistently found that the transductions of different traits were almost entirely independent of each other. Much the same conclusion has emerged from the transformation studies on the pneumococcus and influenza bacillus. If this generalization holds for your experiments, the following test should be possible. Extracts from tumor cells which also carried a second distinctive genetic marker should also transduce neoplasia independently of the second marker, whereas cellular transfers would, of course, carry over every feature of the donor cells. One could, for example, obtain a strain of leukemic cells in which a mutation for resistance to folic-anâagonists had been selected (Law, Nature 169,628 [1952]). Extracts from such cells might be expected to transduce leukemia, but the resulting strain of cells should be sensitive to the folic-antagonists. Other markers may be available as well, but of the most obvious, serological differences might be expected to introduce unwonted complications.

I fully appreciate that this kind of experiment may be impracticable, but would appreciate hearing your reactions to it, if only as an academic exercise. Perhaps you have already considered such a genetic approach. I can only hope that we may see a continuing rapprochement between microbiology, pathology and genetics.

Yours sincerely,

10/13/52 –
Re Klein

Joshua Lederberg
Associate Professor of Genetics